

## VOODOO CRIMINOLOGY AND THE NUMBERS GAME

Cultural criminology is of importance because it captures the phenomenology of crime – its adrenaline, its pleasure and panic, its excitement, and its anger, rage, and humiliation, its desperation and its edgework. I wish to argue that cultural criminology not only grasps the phenomenology of crime but, for that matter, is much more attuned to phenomenology of everyday life in general in the late modern era. We are confronted at this moment with an orthodox criminology which is denatured and desiccated. Its actors inhabit an arid planet where they are either driven into crime by social and psychological deficits or make opportunistic choices in the marketplace of crime. They are either miserable or mundane: they are digital creatures of quantity, they obey probabilistic laws of deviancy – they can be represented by the statistical symbolism of lambda, chi and sigma, their behaviour can be captured in the intricacies of regression analysis and equation.

The structure of my argument is that, given human beings are culture-creating beings and are endowed with free will, albeit in circumstances not of their own making, then the *verstehen* of human meaning is, by definition, a necessity in any explanation of human activity, criminal or otherwise (see Ferrell, 1997). It is in late modernity that such creativity and reflexivity becomes all the more apparent yet, and here is irony: it is at precisely at the time of the cultural turn that a fundamentalist positivism occurs within the social ‘sciences’ with increasing strength and attempt at hegemony.

Let us first of all examine the intimate links between late modernity and cultural criminology. The late modern period is characterised by disruption of employment, of marital stability, of greater spatial mobility, by a pluralism of contested values, by the emergence of mediated virtual realities and reference points, and by the rise of consumerism. It embodies two fundamental contradictions: firstly a heightened emphasis on identity in a time when lack of

social disembeddedness serves to undermine ontological security; and secondly by a stress on expressivity, excitement, and immediacy, at a time when the commodification of leisure and the rationalisation of work mitigates against this. This is a world where narratives are constantly broken and re-written, where values are contested, and where reflexivity is the order of the day (see Young, 1999). For all of these reasons a criminology which stresses the existential, which is focused upon subcultures of creativity and style, which emphasise the adrenalised excitement of human action, on one hand, and the tedium and commodification on the other, goes with the grain of everyday life. Moreover, and counter to claims to the contrary (see Garland, 2000), rational choice/routine activities theory does not fully mirror the texture of the time, but only one part of it: the mundane, the denatured, the mediocre. Let us start by looking at the situation of late modernity.

## **THE LOOSENING OF THE MOORINGS**

This process of boundaries dissolving and boundaries being built up like children's sandcastles on a beach, is characteristic of late modernity. At the same time there becomes an increasing awareness of the social construction of boundaries and their contested nature. That is, any sense of the absolute, the reified, the natural, becomes exceedingly precarious. In this process people become more aware of their own role as actors in society. For, although the existential condition and the creation of human meaning has always been part of what we mean by social, this certainty becomes all the more apparent in late modernity. Why is this?

(a) **Voluntarism** : on the back of the movement towards flexible labour and the modern consumer society, with a myriad of choices, an individualistic society arises where chance, expressivity, meaningful work and leisure becomes an ideal. The American Dream of the post-war period, with its stress on taken-for-granted ends of material comfort becomes overtaken by a new First World

Dream, where meaning and expression are paramount and where lifestyles are to be created. Finding yourself becomes more important than arriving.

- (b) **Disembeddedness** : the flexibility and mobility of labour and the increased instability of the family result in people's lives becoming disembedded from work, family and community. This identity does not immediately and consistently present itself. The irony, then, is that just as there is a greater stress on creating one's identity, the building blocks of identity become less substantial. Furthermore, in a lifetime of broken narratives, constant re-invention becomes a central life task.
- (c) **Pluralism and Contest** : the increase in emigration creates a world which is more pluralistic in values just as the plurality of lifestyles generated by the new individualism. People are, therefore, presented with a social world where values are contested and where there are alternatives of appropriate behaviour and aspiration.
- (d) **The Narcissism of Minor Differences** : Freud, in *Civilisation and its Discontents* (1929), was concerned with how often minor differences between people caused more discomfort than considerable differences. Writers such as Michael Ignatieff and Anton Blok have taken this idea and transferred it to understanding cultural conflicts. Ignatieff, for example, in *Warrior's Honor* (1999). Without needing to adopt a Freudian metaphysic (as Ignatieff does), it is possible to take the nub of the idea and interpret it quite simply in terms of human reflexivity. Cultures which are dramatically different do not present us as possible choices to guide our own behaviour and are much easier to be viewed as the accoutrements of people who are essentially different from us. Cultures which are of minor or slight difference present themselves as possible choices, they represent a disquieting possibility that our own cultural choices are incorrect and, at the very least, not simply the natural behaviour which any 'normal' person would exhibit (see Young, 1999, pp.180-2).

Cultural globalisation via the mass media and mass tourism brings cultures together, narrowing differences both between nations and within nations. The

narcissism of minor differences, therefore, becomes all the more apparent in the late modern world and generates not only a sense of choice rather than determination and inevitability, but also as cultures experience cross-over and hybridisation, a blurring of categories rather than a separateness.

(e) **Mass Media and Virtual Realities** : In late modernity the mass media expands in terms of the percentage of time of a person's life that it takes up – in England and Wales, television and radio alone take up an extraordinary 40% of the average person's waking life, or 60% of the free time of those in work. The media overall becomes more multi-mediated, diversifying and relating to wider audiences (see McRobbie and Thornton, 1995). As the physical community declines, the virtual community arises, carrying with it virtual realities with new and emerging role models, subcultures of value, vocabularies of motive and narratives both fictional and 'factional'. In particular, the two 'others' are polarised: positively the celebrity, and the negative 'others', particularly around the notion of an underclass. Powerful positive and negative orientation points are thus available, various and particular, for the mass of people whose personal narratives have constantly to be rewritten and reinvented. Thus identities are invented and desired at the same time as stable identity is destabilised and choices and reference points offered in plenty.

Thus, Zygmunt Bauman contrasts the post-war modern world with the late modern world of liquid modernity. In the former there were:

“patterns, codes and rules to which one could conform which one could select as stable orientation points and by which one could subsequently let oneself be guided, that are nowadays in increasingly short supply. It does not mean our contemporaries are guided solely by their imagination and resolve and are free to construct their mode of life from scratch and at will, or that they are no longer dependent on society for the building materials and design blueprints. But it does mean that we are presently moving from an era of pre-allocated 'reference groups' into an epoch of 'universal comparison', in which the destination of individual self-constructing labours is endemically and incurably underdetermined, is not

given in advance, and tends to undergo numerous and profound changes before such labours reach their only genuine end: that is, the end of the individual's life.

"These days patterns and configurations are no longer 'given', let alone 'self-evident'; there are just too many of them, clashing with each other and contradicting one another's commandments, so that each one is stripped of a good deal of compelling, constraining powers." (2000, p.7).

It is in such a world that the taken-for-granted world which Alfred Schutz depicts as the 'epoché of the natural attitude' (1967) becomes changes to one of reflexivity, what I have termed a phenomenological epoché (1999, pp.97-98), where the social construction of the world becomes apparent, where social reality is up for grabs and reinterpreted from a myriad standpoints, where contest, blurring and change is the order of the day. For we live, as the sociologist of religion, Helmut Schelsky (1957) observed, in a world of 'permanent reflectiveness' and talks of 'a market in worlds'. This, then, is a world of pluralism, of choice, of difference – although sometimes of a minor nature – a world where human subcultures are in flux. The cultural turn, therefore, follows subcultural theory in viewing culture in the anthropological sense as attempts to solve the problems which people face at particular points of the social structure and emphasises the creative nature of culture. It holds with Bauman the stress on culture's 'endemic restlessness and inborn inclination to transcendence' (Bauman and Tester, 2001, p.33), and stresses the particularly restless and creative nature of late modernity. But, of course, this is not all of the story. As I have documented elsewhere (1999, 2003), the insecure and disembedded nature of identity formation in the present period can easily lead to choices of bad faith. That is, embracing identities grounded in fundamentalism, in gender, or 'race', in nation and in traditional culture which are conceived of as fixed and determined essences. For, as we have seen, in liquid modernity boundaries dissolve only to be re-erected again. Yet for all their reaction against the voluntarism and change of later modernity, even these adaptations – the most lowly and least controversial being the embracing of habit, of creating a cocoon

of unquestioning regularity in one's life (what Merton termed 'ritualism') – these too are choices and social inventions.

Such changes accompanying the cultural turn have extraordinary implications for sociology, particularly for social explanation, but also for measurement and research practice. Let us note at this juncture the significant changes in circumstances of identity formation and with it the vocabulary of motives associated with given roles and structural positions. For the combination of the ideal of choice, disembeddedness and pluralism engenders a situation where vocabularies of motives begin to lose their fixed moorings in particular parts of the social structure and in specific social circumstances. That is, the old rigid moorings of Fordism, the demarcations of class, age and gender, the concentric demarcations of the Chicago model of space in the city begin to dissolve. Vocabularies of motive become loose and cast adrift from their structural sites; they can shift and be fixed elsewhere. This is not willy nilly: there obviously has to be some fit between structural predicament and subcultural solution, but the level of determination and predictability diminishes. Furthermore, they can be bricolaged elsewhere in the system: they can be reinterpreted, transposed and hybridised. And, finally, and most crucially of course, they can be changed and innovated sometimes dramatically.

Let us, for a moment, look at the relationship between material and social predicaments, identity, vocabularies of motive and social action taking crime as an example (although we could as well focus, say, on educational achievement or sexual behaviour). There is an extraordinary tendency to suggest that the motive to commit crime springs fully fledged out of certain material predicaments (eg poverty, unemployment) or social circumstances (eg lack of control) or biological [characteristics] (eg youth and masculinity), almost as if no connecting narrative or human subjectivity were necessary (see Katz, 2002). In reality, a situation like poverty will result in totally different assessments and responses dependant on the narratives which the subjects use to interpret their predicament

– indeed the very assessment of whether one is poor or not will depend on social interpretation. There can be no causality in society without reference to meaning, and even high correlations – as all the methods textbooks tell us – do not necessitate causality (see Sayer, 1992). What is necessary is to understand that we live in a situation where such meanings change rapidly and do not adhere fixedly to particular social roles, or material predicaments. We can separate out two facets of this process: liquidity and change.

Identities carrying with them vocabulary of motives, ideals, reference groups, frustrations, pride and grievance can unravel more readily across society and space. Thus, for example, reference groups can be distant – even virtual – and, more importantly, increasingly universal in their providence. For example, as Bottoms and Wiles posit, teenagers in Britain can have as reference points beach life in Sydney, or as Donna Gaines found out in *Teenage Wasteland* (1998), her study of teenagers in New Jersey, the vocabulary of motives and lifestyle surrounding dropout from school could travel across class and have as key references Heavy Metal groups, some like the British group Led Zeppelin, who were not only orientation points from across the Atlantic, but ‘retro’ in time. Indeed, studies of middle class delinquency frequently show how working class norms spread up the structure (see H and J Schwendinger, 1985; Currie \_\_\_\_). The paradigm case of this is, of course the rise of hip hop music and rap lyrics from the black ghettos and its rhetoric of respect, rejection, and challenged masculinity having huge audiences amongst white youth of all class backgrounds. On a different level, the universal values of feminism, arising originally out of middle class white feminists, have now a purchase which resonates across the world: from Iran to Bradford, and whose parameters of freedom and equality become a key benchmark in so many women’s lives. In the realm of crime motivation, style and even modus operandi are globally connected: a serial killer from Singapore gains narrative from American novels, a Yardie in Brixton, London, picks up notions of gangsterdom from *The Godfather*, as indeed to the American Mafiosi themselves. All of these concepts

travel across time and place, structure and space, disseminated widely by a prolific and insistent media, yet, of course, all change in the process: hybridise and cross over as they are transposed between groups. But the close fit between motives and structure, the more localised and specific responses, become weakened with the extraordinary power of the mass media, coupled with the undercutting of embeddedness in locality and the mass nature of cultures characteristic of the modernism of the post-war period.

Secondly, let us look at change. Late modernity has been characterised by extraordinary changes in the major social sites of class, youth, gender, and ethnicity. Indeed, Eric Hobsbawm, in his *Age of Extremes* the history of

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Concepts of what it is to be young, what it is to be female, what pleasures we should expect, our attitudes to work, sexuality, and leisure have all been dramatically recast. I will concentrate on a few selected examples of such change – but first of all let me point out what is perhaps – *or should be* – the obvious. None of these changes were predictable from the social, economic and cultural ‘variables’ present before these fundamental roles and values were reinvented. They are understandable, in retrospect, in terms of responses to material and social change, but they were re-fashioned by human actors who simply re-wrote their narratives.

Edward Shorter, in his magisterial *The Making of the Modern Family* (1975), points to the two major transformations in the late modern family being the greater freedom of women and the loss of control over adolescents. The advances and acceptance of contraception, plus the extension of life span, changed the historical role of women dominated by childbirth and childcare throughout their adult lives. This allowed the development of working lives and gave rise to perhaps the most important structural change of late modern times,

the entry of women into the labour force and greater female independence. This resulted, he argues, in much greater levels of family instability, rise in divorce rates, etc. To this one might add: that this fundamental structural change generated second wave feminism, one of the most influential intellectual currents on a global scale of the late modern world, the evolution of the concept of the dual role, of equality. Furthermore, of immediate interest to the argument of this article: the movement of women out of the family gave rise to a feminisation of the public sphere, a rising intolerance of violence in all spheres of life. It has involved characteristically a growth of intolerance of violence in public life and the widespread denunciation of violence in private life (eg wife battering, child abuse, rape, sexual abuse, etc.).

As for adolescents, the availability of work or of extended education involves. Shorter argues, for their intensive socialisation outside of the family by their peer group. Let us add to this such a movement of youth into the public sphere, paralleling the greater autonomy of women, allows for a group with some degree of financial independence, complemented with greater sexual freedom because of contraception, often frustrated in their desires for material goods and expressive freedoms, to enter the arena in terms of a new creation: the dissatisfied and often rebellious teenager. And, of course, the teenage 'revolution' was one of the great changes of the late twentieth century – something so dramatic that, as so often with such sweeping changes, we can scarcely see it now.

Here again let me restate the obvious. No one knew what youth were going to do with their new position and status. If you were attempting to predict crime rates from the 'variables' which seemed to explain crime rates in the fifties, one would have talked about inequalities, employment levels, educational achievement, percentage of adolescent males in the population, divorce rates, etc. But even the most sophisticated statistical analysis (very unlikely at that

time) could not have predicted the extent of youth crime and the reason for this is palpably simple: you could not have anticipated what was to happen to 'youth'.

Let me now turn to the problem of measurement, and here I will repeat the structure of my argument. First I will note how the social and meaningful nature of human action make positivistic methods inappropriate, second I will indicate how the situation of late modernity heightens this situation. To do this I will turn first of all to a debate within the sociology of sex.

### **MEASUREMENT AND THE SEXOLOGISTS**

In April and May 1995 the columns of *The New York Review of Books* were subject to a remarkable and, some would say, acrimonious debate. It was an argument which was, to my mind, one of the most significant examples of academic whistle-blowing, wide ranging in its critique, apposite in its target and reasoning, timely and badly needed yet falling, as we shall see, on stony ground.

On one side of this skirmish was Richard Lewontin, Professor of Zoology at Harvard, a distinguished geneticist and epidemiologist, on the other a team of sociologists led by Edward Laumann, John Gagnon, from the University of Chicago, who had recently published *The Social Organisation of Sexuality* (1995), and its popular companion volume *Sex in America: A Definitive Survey* (1995). On the sides, chipping in with great gusto, Richard Sennett, joint Professor of Sociology at the LSE and NYU – one of the leading sociologists of our time.

This debate is of interest because it represents the direct contradiction of natural science with sociology or social science, as it is often hopefully and optimistically called. Such encounters are relatively rare and tend to occur when particularly politically distasteful findings are presented to the public as cast iron and embellished with the primatur of science. A recent example of this was Richard Herrnstein and Charles Murray's *The Bell Curve* (1994), accompanied by pages

of statistical tables which purported to present the scientific evidence for the link between race, IQ and, indeed, crime. At that time many prominent scientists, including Steven Rose and Stephen Gould, were moved to intervene, but normally the walls between disciplines remain intact: indeed a collegial atmosphere of mutual respect coupled with lack of interest ensures that parallel and contradictory literatures about the same subject can occur in departments separated sometimes by a corridor or, more frequently, a faculty block. In the case of the natural and social sciences this is complicated by a unidirectional admiration – a one-sided life, one might say – or at least a state of acute physics envy – between the aspiring social scientist and the natural sciences. Be that as it may, a considerable proportion of sociologists, the vast majority of psychologists and an increasing number of criminologists embrace, without thought or reservation, a positivistic path. Namely, that natural scientific methods can be applied to human action, that behaviour is causally determined, that incontestable objectivity is attainable and that precise quantitative measurement is possible, and indeed preferable. In the case of criminology, this entails the belief that the crimes of individuals can be predicted from risk factors and that rates of crime can be explained by the changes in the proportion of causal factors in the population.

Richard Lewontin sets out to review the two books. They arose on the back of the AIDS crisis and the need to understand the epidemiology of its spread. The survey was eventually well funded by the research foundations, was conducted by NORC, the premiere social survey research organisation in North America. The project was initiated by the National Institute of Health on behalf of a coalition of Federal agencies concerned about AIDS. For a project which, as we shall see, was beset by problems of concealment, it is of ironic interest that, in the search for funding, the title of the project itself deliberately avoided mentioning sex, entitling itself: 'The National Health and Social Life Survey', whilst the original proposal issued by the National Institute of Health was entitled: 'Social and Behavioral Aspects of Fertility-Related Behavior'. 'At very least', as

Lewontin comments, 'there is some anatomical confusion here' (p.25). Funding was then refused by the Bush (I) administration until eventually rescued by the Foundations.

The project involved a sample of 3,432 people representing 200 million post-pubertal Americans. Just for a minute let us think of the audacity of the sample survey – and this one more thoroughgoing than most – the claim to generalise from such a small number to such a large population of individuals. Today we take for granted in public opinion polls, in market research, and in social scientific work, the facility of the social survey. They are the staple of political debate and social commentary, yet their basis is much more shaky than we often assume and it is for this reason that Lewontin's critique is of much greater purchase than a commentary on sex studies or criminology for that matter. Lewontin's criticism proceeds on two levels, one the problem of representativeness and two – and more substantially – the problem of truth. Let us first of all examine the problem of representation. An initial criticism is that the random sample was not actually from the total population. It is based on a sample of addresses drawn from the census, but it excluded households where there were no English speakers, nor anyone between the ages of 15-59. most crucially it excludes the 3% of Americans (some 7.5 million) who do not live in households because they are institutionalised or are homeless. This latter point is, as Lewontin indicates, scarcely trivial in understanding the epidemiology of AIDS as it excludes the most vulnerable group in the population, including those likely to be victims of homosexual rape in prison, prostitution, reckless drug use, the sexually 'free' college aged adolescents etc. The random sample is not, therefore, drawn from the population as a whole: a *very* atypical population is omitted. Such a restriction in population sampled is a usual preliminary in survey research

However, once this somewhat restricted sample was made, the research team did not stint in their efforts to get as large a response rate as possible. After repeated visits, telephone calls and financial inducements ranging from \$10 to

\$100, the result was a response rate of 79% - of which they were duly pleased. But, as Lewontin points out: 'It is almost always the case that those who do not respond are a non-random sample of those who are asked' (1995a, p.28). In this case it could well be prudishness, but in the case of other surveys equally non-random causes of non-responses. For example, in our own experience of over fifteen large-scale crime and victimisation studies which we ran at The Centre for Criminology, Middlesex University (see for example Jones *et al*, 1986; Crawford *et al*, 1990) we made every effort to reduce non-response but never managed better than 83%. Indeed, criminal victimisation surveys as a whole have between one fifth to a quarter of respondents whose victimisation is unknown. As I remarked at the time, in the thick of quantitative research:

'It goes without saying that such a large unknown population could easily skew every finding we victimologists present. At the most obvious level, it probably includes a disproportionate number of transients, of lower working class people hostile to officials with clipboards attempting to ask them about their lives, and of those who are most frightened to answer the door because of fear of crime.' (Young, 1988, p.169).

Even panel studies which follow a given population over time suffer from this problem. To take the famous Seattle Social Development Project as an example (see Farrington *et al*, 2003), which is a prospective longitudinal survey of 808 children. To begin with, these are only the children/parents who consented to be included out of the population of 1,053 fifth-grade students targeted – that is, it has a 70% response rate from the outset – with 30% refusing consent. Secondly, youths dropped out over time so, for example, by the age of 12 the sample fell to 52%. There is, of course, every reason to suspect that those who initially did not consent and those who fell out of the panel might have different delinquency patterns to those who consented and remained within the panel.

Lewontin's first point – the problem of representativeness – is, therefore, clear and is as applicable to criminology as to other fields of sociology. Let me at this point remind the reader of Quetelet's warning. Adolphe Quetelet, the founder of

scientific statistics, and a pioneer in analysing the social and physical determinants of crime, introduced into academic discussion in the 1830s the problem of the unknown figure of crime. That is crime not revealed in the official statistics:

'This is also the place to examine a difficulty ... it is that our observations can only refer to *a certain number of known and tried offenders out of the unknown sum total of crimes committed*. Since this sum total of crimes committed will probably ever continue unknown, all the reasoning of which it is the basis will be more or less defective. I do not hesitate to say, that all the knowledge which we possess on the statistics of crimes and offences will be of no utility whatsoever, unless we admit without question *that there is a ratio, nearly invariably the same, between known and tried offences and the unknown sum total of crimes committed*. This ratio is necessary, and if it did not really exist, everything which, until the present time, has been said on the statistical documents of crime, would be false and absurd.' (A Quetelet, 1842, p.82)

Quetelet's fixed ratios are, of course, a pipe dream, as unlikely as they would be convenient. His warning, written in 1835 (English translation 1842) has echoes throughout the criminology academy for the last one hundred and seventy years. If we do not know the true rate of crime all our theories are built on quicksand. They will be of 'no utility', 'false', and indeed 'absurd'. Legions of theorists from Robert K Merton through to James Q Wilson have committed Giffen's paradox: expressing their doubts about the accuracy of the data and then proceeding to use the crime figures with seeming abandon, particularly in recent years when the advent of sophisticated statistical analysis is, somehow, seen to grant permission to skate over the thin ice of insubstantiality (Giffen, 1965; Oosthoek, 1978). Others have put their faith in statistics generated by the social scientist, whether self-report studies or victimisation surveys, as if Quetelet's warning no longer concerned them and the era of pre-scientific' data was over.

Indeed, Richard Sparks and his associates, in the introduction to their groundbreaking London victimisation study, summarised the decade of American research prior to their own with a note of jubilation: 'Within a decade ... some of

the oldest problems of criminology have come at least within reach of a solution.’ (Sparks *et al*, 1977, p.1). As we have seen, the problem of non-response means that such a resolution of the age old problem of measurement is not resolved. It would be so, of course, if the non-respondents were just – or almost – like the respondents and indeed such an excuse is often invoked with as much likelihood of validity as Quetelet’s ratios. As it is, the atypicality of non-respondents is likely to overturn the significance levels of any probabilistic sampling. Richard Sparks was quite clear about this in his assessment of the potential of victimisation studies. His initial excitement became tempered by considerable caution. Thus he writes, three years later:

‘Much too much fuss is made, in practically all official NCS publications, about statistical significance (i.e. allowance for sampling variability). A variety of standard errors and confidence intervals for NCS data are now routinely quoted in those publications. Yet it is clear that *nonsampling* error is of far greater magnitude in the NCS; adjustments ... may offset some of this nonsampling error, though only in a ballpark way, which makes questions of sampling variability virtually irrelevant.’ (1981, p.44, n.42)

### **TELLING THE TRUTH?**

But let us go on to Lewontin’s next criticism: the problem of truth. And here the problem is even more important and substantial than that of non-response and the dark figure. This revolves around the key question of whether those who responded to the questionnaire were in fact telling the truth. That is, that social surveys may not only have dark figures of non-respondents, but a dark figure of non-response – and indeed sometimes ‘over-response’ – amongst the respondents themselves.

It is rare for surveys of attitude or self-reported behaviour to have any internal check as to validity. After all, if people say they would rather live by work than on welfare, if they profess liberal attitudes on racial matters, or if they tell you that they were assaulted twice last year, how is one to know that this is not true? One may have one’s suspicions, of course, but there are few cast iron checks.

Every now and then, however, anomalies stare you in the face. In the case of this sexual behaviour survey there is a particularly blatant example. For the average number of heterosexual partners reported by men over the last five years is 75% greater than the average number reported by women. This is an obvious anomaly, it is, as Lewontin points out, like a violation of the only reliable law in economics: that the number of sales must be equal to the number of purchases. Furthermore it turns out that such discrepancies are well known in sex studies. For on the back of the panic over AIDS, parallel studies occurred across the Western world and all of them found a wide differential between the number of sexual partners reported by men and women<sup>1</sup>. Interestingly the gap is greater for some countries rather than others – French men, for example, would seem to have more discrepant claims than Americans or Britons. Such claims in numbers of sexual partners and frequency of sexual intercourse are very obviously a function of male boasting, female modesty and indeed different definitions as to what actually constitutes sexual intercourse. They tell us something about the differences between the sexes, something about different levels of double standards in various countries and over time, but only very obliquely and obscurely about different rates of sexual behaviour. Ironically, such discrepancies were well known to early sex researchers, such as Kinsey, which rather smugly the US researchers claim to have easily surpassed in their sophistication.

What is startling is that the researchers are well aware of this. Indeed the US researchers devote considerable time to debating which of several reasons might have caused this ‘discrepancy’ and conclude that the most likely explanation is that ‘either men may exaggerate or women may underestimate’. As Lewontin remarks,

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<sup>1</sup> For France see ACFS, 1992; for Britain, Evans *et al*, 1989; for Sweden, Herlitz, 1990. for an insightful critical commentary, see C Francome, 1993; 1986. The controversy over sexual behaviour figures is reflected in the arguments between Gay rights groups and researchers in Britain over the latter’s claims that there are far fewer gay men than is usually assumed. Gay activists, quite correctly, point to the likelihood of widespread concealment in the survey situation (see Katz, 1994).

'So, in the single case where one can actually test the truth, the investigators themselves think it most likely that people are telling themselves and others enormous lies. If one takes the authors at their word, it would seem futile to take seriously the other results of the study. The report that 5.3 percent of conventional Protestants, 3.3 percent of fundamentalists, 2.8 percent of Catholics, and 10.7 percent of the non-religious have ever had a same-sex partner may show the effect of religion on practice or it may be nothing but hypocrisy. What is billed as a study of "Sexual Practices in the United States" is, after all, a study of an indissoluble jumble of practices, attitudes, personal myths, and posturing.' (1995a, p.29)

What is of interest here is the awareness of thin ice, yet the ineluctable desire to keep on skating. Just as with Giffen's paradox, where the weakness of the statistics is plan to the researchers yet they continue on to force-feed inadequate data into their personal computers, here the problem of lying, whether by exaggeration or concealment, does not stop the researchers, for more than a moment, in their scientific task. Of course, as a sociologist, such findings are not irrelevant: they inform you much about differences in male and female attitudes to sex – what they *don't* tell you is about differences in sexual behaviour. Yet what Richard Lewontin is telling us is that the interview situation is a social relationship – that results are a product of a social interaction and will vary with the gender, class, and age of the interviewer and of the interviewee. But here we have it: it needs a Professor of Biology to tell sociologists to be sociologists. Thus, he concludes:

'The answer, surely, is to be less ambitious and stop trying to make sociology into a natural science although it is, indeed, the study of natural objects. There are some things in the world that we will never know and many that we will never know exactly. Each domain of phenomena has its characteristic grain of knowability. Biology is not physics, because organisms are such complex physical objects, and sociology is not biology because human societies are made by self-conscious organisms. By pretending to a kind of knowledge that it cannot achieve, social science can only engender the scorn of natural scientists and the cynicism of humanists.' (*ibid*, p.29)

Of course this is not the end of it. Edward Laumann and his colleagues are outraged. They do not think it 'appropriate for a biologist ' to be reviewing their work, he does not have the right 'professional qualifications' – 'his review is a pastiche of ill-informed personal opinion that makes unfounded claims of relevant scientific authority and expertise' (1995b, p.43).

Lewontin, in reply, notes caustically that it is understandable that the team

'would have preferred to have their own work reviewed by a member of their own school of sociology, someone sharing the same unexamined methodological assumptions. They could avoid the always unpleasant necessity of justifying the epistemic basis on which the entire structure of their work depends.' (1995b, p.43)

As to his incompetence with regards to statistical analysis, he points to being a bit disturbed to have to reveal his CV, but that he has a graduate degree in mathematical statistics which he has taught for forty years and this is the subject of about one-tenth of his publications including a textbook of statistics!

Lewontin returns to the strange belief that positivist sociologists have that there is no social relationship involved in the process of data collection:

'Is it really true that quantitative sociologists are so divorced from introspection and so insensitive to social interactions that they take such a naïve view of human behavior? Do they really believe all those things they hear from the person on the next bar stool or the seat next to them in the airplane? The yellow Kid, who made a living from fleecing the gullible, used to say that anyone who could not con a banker ought to go into another line of work. Maybe, but before giving up, they should try professors of sociology. Putting aside subjective questions, haven't they even read the voluminous literature on the sociology of fashion? It is ironic that a student of "simple organisms" has to instruct those who inquire about human beings about the complexity of their objects of study.

... people do not tell *themselves* the truth about their own lives. They need to create a satisfying narrative out of an inconsistent and often irrational and disappointing jumble of feelings and events leads each of us to write and rewrite our autobiographies inside our own heads, irrespective of whether anyone else is ever privy to the story. Second, those stories,

which we then mistake for the truth, become the basis for further conscious manipulation and manufacture when we have exchanges with other human beings. If the investigators at NORC really do not care what strangers think of them, then they are possessed of an insouciance and hauteur otherwise unknown in Western society. It is precisely in the interaction with strangers who are not part of their social network, and who will never intersect their lives again, that people feel most free to embroider their life stories, because they will never be caught out.' (1995b, p.44)

And, of course, such a process of believing in the objectivity of data is fostered by the habit of researchers of *not* conducting their own interviews, of employing agencies such as NORC, or second-hand, in terms of using older datasets or even a meta-analysis of past datasets. So the data arrives at their computers already punched, sanitised: it is a series of numbers with scientific-looking decimal points. Human contact is minimised and a barrier of printout and digits occurs between them and human life.

But let us leave the last remarks of this section to Richard Sennett. He congratulates Lewontin on the brilliance of his analysis, he laments the current fashion of scientific sociology, and concurs with Lewontin's remark that, if work such as this is typical, then the discipline must be in 'deep trouble'. That's putting it mildly, suggests Sennett, 'American sociology has become a refuge for the academically challenged' (1995, p.43). But he adds that mere stupidity itself cannot alone explain the analytic weakness of such studies for 'sociology in its dumbed-down condition is emblematic of a society that doesn't want to know much about itself' (*ibid*).

## **LESSONS FOR CRIMINOLOGY**

But what has all of this to do with criminology? A great deal and more, for it is probably criminology, of all the branches of sociology and psychology, where the problem of unchecked positivism is greatest. The expansion of academic criminology was a consequence of the exponential increase in the size of the criminal justice system just as the shift from students studying social policy/

administration to criminology parallels the shift from governmental interventions through the welfare state to those utilising criminal justice. The war on crime followed by the war on drugs and then on terror. This has been accompanied by an expansion in funding designed to evaluate and assess governmental interventions and programmes. The material basis for the revitalisation of positivist criminology is considerable and, certainly within the United States, approaching hegemonic. (see Hayward and Young, 2004)

Let us examine some of the discrepant findings which have occurred in recent criminology.

### **Embarrassing Findings**

We have already examined the *technical* problems of the dark figure in crime statistics, whether in police figures, self-report data, or victimisation studies. Let us now turn to the second *substantive* problem: that of concealment, or exaggeration. Criminological research is replete with findings which range from the very unlikely to the ridiculous. I will give just a few examples:

#### ***The Education Effect***

One of the most widely recognised anomalies is victimisation research arising out of 'the education effect'. This is where higher reporting of victimisation is linked to educational attainment and is, in Richard Sparks' (1981) estimation, one of the 'most serious' response biases. The education effect, first spotted by Biderman in his survey for the 1967 President's Commission (Biderman *et al*), it occurred in Sparks' and his colleagues London survey (Sparks, Genn and Dodd, 1977) and was found with regards to violence in *The British Crime Survey* (Hough, 1986. See commentary Skogan, 1986; Coleman and Moynihan, 1996).

It appears an obvious anomaly in The National Crime Survey where, for example, the 1977 report shows reported assault generally to rise with educational attainment so that, for example, for white respondents those with

some college education had twice the rate of aggravated assault as those who had only completed eighth grade and for black respondents a similar pattern appeared. Sparks stresses the anomalous nature of such findings:

‘One of the most serious of these biases concerns the almost certain effect on survey responses of social-class-linked variables, in particular educational attainment. “Being a survey respondent” is, in many ways, a middle-class game; it requires a certain amount of verbal fluency and a capacity for abstract conceptualization, both of which are to some extent concomitants (if not consequences) of formal education. In a victimization survey, the tasks which a respondent must perform involve casting one’s mind back over a particular period in one’s past and fitting descriptions given by the interviewer’s questions (“Did anyone break into your house?”) to the events which may have occurred in that time. It would not be surprising to find that these classroom-like tasks would be better performed by those with more practice (in the classroom) at them; and there is increasing evidence that this is precisely what happens.’ (Sparks, 1981, p.34)

But he goes further than this, noting that such an education effect will ‘infect’ much of the survey data and, of course, let us add that this would be all the more insidious as no obvious anomaly could be observed in these presumably numerous instances:

‘This bias is all the more serious in view of the strong associations which exist in contemporary American society between educational attainment and other demographic and social variables which are important both for criminological theory and for public policy concerning crime: that is, variables such as age, sex, race, income, area, and region of residence. Thus, for example, blacks, older persons, females, lower-income persons, southerners, and rural residents are, on average, less educated than their counterparts; the interrelations between these things, and the effects of those interrelations on educational attainment, may be extremely complicated. A response effect due to educational attainment is thus likely to infect pretty well any inference about victimization which might be made from NCS data.’ (*ibid.*, p.34)

### ***The Assault Rate on White Men***

A similar finding for the above occurs in terms of assaults and ethnicity. The United States NCVS regularly comes up with results which show that the assault

rate reported by white men is just about that of black men. For example, in 1999 the rate was 32.3 per 1,000 for whites compared to 31.0 per 1,000 for blacks. (Maguire and Pastore, 2001) This is totally against any evidence from homicide rates or other indices of violence (see Sparks, 1981, for similar earlier findings).

### ***Rarity of Serious Crimes***

Victimisation studies consistently report levels of serious crime which are gross underestimations and are *freely admitted* as such. For example, the first British Crime Survey of England & Wales in 1982 found only one rape and that attempted (see Hough and Mayhew, 1983).

### ***Variability of Findings with Different Instruments***

If we take sensitive topics such as incidence of domestic violence, the range of figures are extraordinarily wide – and, in no doubt, underestimates. Thus in 1996 the percentage of women experiencing domestic violence, defined as physical assault with injury, was 0.5% in the police figures, 1% in the British Crime Survey, and 2.2% when Computer Assisted Self-Interviewing was used. An independent survey found a rate in the region of 8% (see Mooney, 2000). Which figure in this range is one going to feed into one's PC? What sort of science is it where a variable varies sixteen fold?

### ***Self-Report Studies***

Consistently come up with results showing that there is little variation between the levels of juvenile delinquency between the working class and the middle class, between black and white, and produce a considerably reduced gap between males and females. Hence Charles Tittle and his associates (1978) extraordinary claim that there is no relationship between class and delinquency. What is even more remarkable is that influential theorists, particularly Travis Hirschi and his associates, have built theories around such a rank implausibility (see Gottfredson and Hirschi, 1990 in their grandly entitled *A General Theory of Crime*). As David Downes and Paul Rock point out: 'Since control theorists make so much of the strength of their case empirically, it is worth noting that in

certain respects Hirschi's data strain credibility – a pleasant change from theories producing that effect.' (1988, p.237n).

There can be no better example of the fetishism of data than the rejection of the relationship between social class and conventional crime. If one is talking of *all* crime and includes corporate and white collar crimes this is, of course, a different matter, but this is not the focus of self-report studies nor the theories emanating from them (see Currie, 1985). All one can record about this surmise is John Braithwaite's pithy remark:

'it is hardly plausible that one can totally explain away the higher risks of being mugged and raped in lower class areas as the consequence of the activities of middle class people who come into the area to perpetrate such acts.' (1981, p.37)<sup>2</sup>

### ***Findings of the International Crime and Victimization Studies***

The International Crime and Victimization Study (vanKesteren *et al*, 2000) frequently finds rates of reported violence between nations which are almost the inverse of the homicide rates. In fact, I am surprised that no one has run a Pearson Rank Coefficient on their findings. (see Young, 2004, and discussion in Chapter X).

## **THE PLURALISM OF THE DARK FIGURE**

Up till now we have discussed either technical problems of non-response or the more substantive problem of exaggeration or lying. I want now to turn to a third problem which generates even greater and more impenetrable barriers for scientific quantification. The first two problems – which Lewontin addresses – that of representativeness and truth, presume that there is an objective data

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<sup>2</sup> Gottfredson and Hirschi's riposte to this is *far* from convincing:

'The most common device for "clinching" the argument about social class and crime invites the disputee to park his car in a slum area or to walk the streets of lower-class sections of town and compare the experience with a similar walk in affluent areas. As a rhetorical device, this argument is effective, but it is not necessarily accurate (graveyards also exude fear, although there is some question about whether the people in them are especially dangerous)' (1990, p.80, n.8)

there to be registered. However, there is a profound difference between measurement in the natural world and in society, namely that the definitions of social phenomena are constructed by individuals and in this they will vary with the social constructs of the actors involved. If one hands out a dozen metre rules to students and asks them to measure the length of the seminar room, they will come to a common agreement with a little variation for accuracy. If one asks the room full to students to measure levels of violence they are, so to speak, already equipped with a dozen rules of different gauge and length. They will come out of the exercise with different amounts of violence because their definitions of violence will vary. And the same will be true of the respondents to a victimisation study. All of us may agree that a stab wound is violence, but where along the continuum does violence begin: is it a shove (if so, how hard?), is it a tap (if so, how weighty?), or perhaps it is a harsh word, an obscenity, a threat? People vary in their definitions and tolerance of violence: there is a pluralism of measures. (See Hough, 1986; Young, 1986; Mooney, 2000).

### **[DURKHEIM?]**

Let us look at our 'anomalies' in this light. The peculiar results of the International Crime and Victimisation Studies where the rates of violence reported approximate the inverse of the rates of violence occurring if we are to trust the homicide figures, may well be not only that reporting to strangers distorts the level of violence. It may well be that countries with low levels of violence may well have low levels of tolerance of violence and thus report acts which other, more tolerant/violent nations, might ignore. Similarly the comparatively higher rates of violence against white compared to black men in the United States or the educated compared to the less educated may well reflect differences in definition of what constitutes 'real' violence.

Once we have acknowledged the pluralism of human definition, we can then return to the dark figure with even greater doubts and trepidations. For the dark figure will expand and contract not merely with the technical means we bring to it,

but with the values of the respondents and indeed the categories of the interviewers. And the social rather than the merely technical permeates our measurement on all three levels: whether it is the respondents who refuse to talk to us, to those that in their relationship with a stranger (of class, gender, age, and perhaps ethnicity) will attempt to convey an impression of themselves (a product of their own personal narrative which they have woven around the 'facts' of their lives), to the values and meanings which the interviewee brings to the table.

### **LEARNING TO ANSWER THE QUESTIONNAIRE**

To this notion of differential tolerance to crime, particularly violence, and, therefore, different definitional levels, Sparks adds a further comment:

'One of these biases concerns the almost certain effect on survey responses of social-class-linked variables, in particular educational attainment. "Being a survey respondent" is, in many ways, a middle-class game; it requires a certain amount of verbal fluency and a capacity for abstract conceptualization, both of which are to some extent concomitants (if not consequences) of formal education. In a victimization survey, the tasks which a respondent must perform involve casting one's mind back over a particular period in one's past and fitting descriptions given by the interviewer's questions ("Did anyone break into your house?") to the events which may have occurred in that time. It would not be surprising to find that these classroom-like tasks would be better performed by those with more practice (in the classroom) at them; and there is increasing evidence that this is precisely what happens.' (Sparks, 1981, p.32)

[THIS IS A REPEAT – IT'S ALSO ON PAGE 21]

Thus, to differential tolerance thresholds to violence, we must add different capacities in abstract thinking, a willingness to recall violence and to conceptualise what is often a blur of events as a series of events that can be conceptualised.

### **EYSENCK'S DILEMMA**

It is important to stress how damaging such findings are for the positivist, for the scientific project of studying humanity. For positivism needs fixed categories, agreed measurements, objective and uncontested figures. The late Hans Eysenck, the doyen of psychological behaviourism, recognised this quite clearly in the last book he wrote on criminology with his colleague Gisli Gudjonsson. For, in *The Causes and Cures of Criminality* (1989) they began by taking issue with the authors of *The New Criminology* (I Taylor *et al*, 1973) in their assertion that crime is not an objective category but a product of varying legal fiat. Eysenck and Gudjonsson quite clearly recognise this as an obstacle to science and get round the argument by differentiating two types of crime: victimless and victimful crime. Victimless crimes – and they give examples from prostitution to anal sex – they concede, are subjectively and pluralistically defined. These are eliminated from the realm of objectivity and hence scientific study – but victimful crimes, and here they list such phenomena as theft, assault, murder, rape, are, they argue, universally condemned and, therefore, clearly in the sights of the objective. But this is obviously untrue: all of these crimes are subject to varying definitions – to talk of them having a fixed nature is to teeter on the brink of tautology. Rape is, of course, universally condemned because it is an illegal sexual attack, but what constitutes rape varies and, indeed, expands with time, witness the acknowledgement of marital rape as rape. And assault, as we have seen, is greatly dependent on our tolerance of violence.

### **INSIDER SCEPTICS**

It is of interest that at the point of introduction of a new method of measurement, the authors are often remarkably frank about the limitations of method: it is only later that doubts are expressed as an aside, a momentary hesitancy. Thus, both Richard Sparks Senior, the American pioneer of victimisation research, and Mike Hough, the author of the first British Crime Survey (Hough and Mayhew, 1983) were remarkably candid about their research findings. Let us note at the outset that neither of these men were methodological radicals, both of them represent

orthodoxy, both are diligent social scientists blessed with little epistemological doubt or theoretical wavering.

We have already seen how Sparks was sceptical about the use of tests of statistical significance as the non-response rates would easily skew any figures. But he goes much further than this, for he concludes his survey of victimisation research with the ironic:

‘At this point the reader may well wonder why this essay is subtitled “an optimistic assessment.” Has it really been worthwhile to do fifteen years of extremely expensive research, merely to show that, Yes, Virginia, there really *is* a “dark figure” or unrecorded crime? Is it worth continuing to spend money on a research technique with an error structure of unknown but possibly enormous proportions, which may be producing findings that are not only mistaken but downright misleading?

(There are probably some people who have no difficulty in believing that the college educated are more at risk of aggravated assault than those with an eighth-grade education.) Is the victimization survey technique ever likely to provide information about crime that will be useful to researchers, policy-makers, or administrators of the criminal justice system, and that could not be obtained in other (and less expensive) ways?’ (1981, p.45)

Richard Sparks’ answer to his question is, of course, yes, though he prefaces this with candour: ‘At this point, I suppose I must declare an interest: I have invested about a decade of my professional career in this kind of research, and an investment of that magnitude is not calculated to make one condemn the method as worthless.’ (*ibid.*) But his actual answer to the question is quite limited – gone is the scientific breakthrough and left is a more pragmatic approach. Victimisation research reveals that there is a vast amount of criminal behaviour unknown, it puts police statistics and their limitations in perspective, it provides valuable information for policy makers etc. Here I think he is correct, particularly in terms of policy. For if we reject the notion of figures which can be used scientifically because of their precision and definitions, we can still use such survey research as baseline and ballpark figures. Let me give an example,

where, because of problems of representativeness, truth and plurality of definition we can never produce figures which tell us what the rate of domestic violence is, we can still produce evidence of great social utility. We can produce figures which say, for example, that 15% of the female population has, with the definition of violence suggested in the questionnaire, claimed that domestic violence has occurred against them, that this is no doubt an underestimate, is a composite of different interpretations of what constitutes such violence, yet is a large figure which demands that policy makers take seriously in terms of strategies of intervention and resources. This gives us vague and shadowy figures which can be of great use to the policy maker, but which have always to be interpreted, which are by their very nature blurred, which do not allow the refinement of decimal place, and are not the materials for statistical analysis but belong to a world of 'quite a lot', 'not much', or 'considerable'.

Let us turn to Mike Hough, writing in 1986, and reflecting on the findings of the pathbreaking first British Crime Survey, which he had recently do-directed. His remarks are extraordinarily insightful and serve to take us further in this critique. He points first of all to the various problems of under-reporting which we have already discussed but he argues that he believes there is a clear distinction between the reliability of data about property crime and crimes of violence:

'These problems might be regarded by some simply as sources of measurement error – with the hope that methods can be refined in future. At their root, however is an insurmountable problem. There is not – and could probably never be – any degree of agreement between broad social groups in defining assault, attacks, fights, etc. Critics of crime surveys sometimes argue that definitional problems vitiate the entire project of "counting crime". Definitions of particular crimes, the argument goes, are not absolute but vary across groups. ... This argument does not carry a great deal of weight so far as burglary goes, for instance, or car theft; one can assume extensive, though not total, agreement throughout the population about the sorts of incident which would qualify for a positive response to burglary "screen" questions in a survey. (...) But this is simply

not so for most types of violent victimization. When does pushing, jostling and shoving become assault? And when does common assault turn into a more serious attack? Different people, and different groups of people, will apply different sets of criteria in answering “screen questions” such as “has anyone hit you or attacked you in any way?”.’ (1986, pp.121-2)

Note then his notion of an ‘insurmountable problem’ with regards to violent crime, and his differentiation between types of statistics. His conclusion evokes an interesting parallel with medical statistics:

‘Whilst therefore a well-conducted crime survey may provide a better measure of the number of, e.g. burglaries in an area than do police records, it is hard to sustain the argument that either police records or survey statistics provide an adequate measure of violent offenses. A parallel with medical epidemiology can be usefully drawn. Some illnesses can be counted – with error, no doubt, but they can be counted so as to provide useful information. Thus, in England and Wales health statistics are collated about various forms of cancer, heart disease and the like. Some illnesses, however, cannot and could never be counted with anything approaching accuracy; the best examples come from mental health – depression, for example, schizophrenia or psychosis. It does not make sense to try to count the number of “onsets” of these because there is no real agreement over their definitions.’ (*ibid.*, pp.122-3)

## **THEY KNOW THE ICE IS THIN BUT KEEP ON SKATING ON**

What is remarkable is that the knowledge of the tenuous nature of the statistics is widespread, yet it does not seem to stop the would-be social scientist for more than a minute. Somewhere tucked into the text the author admits the precariousness of their arguments, their scientific vulnerability, and yet continue on. It is as if the skater hesitates, notes the thin ice, yet skates blithely on. Let me give two examples out of the many; to do so is almost invidious, but select we must:

1. *Braine et al Study of Desistance (2003)*

'Laub and Sampson (2001:9) indicate that many difficulties confronting the measurement of criminal desistance also confront the study of individual involvement in criminal activity more generally. This raises important and difficult issues about whether criminal desistance could be studied with official records or self-reports, the appropriate range of behaviors that fit within the concept of "criminal behavior," and whether similar behaviors are interpreted consistently by different social and demographic groups (see e.g., Farrington and West 1995; Nagin et al., 1995; Uggen and Kruttschnitt 1998; Sommers et al., 1994; Warr 1998; Nielsen 1999; Benson 2001). Our objective in this paper is not to resolve these issues but rather to explore the implications of studying desistance through different analytical windows while assuming that the other difficulties in the study of criminal desistance are non-problematic. This approach allows us to focus our attention on variation in our results under different analytic frameworks while holding measurement issues constant.' (Braine *et al*, 2003, p.426, n.6)

What is fascinating about this footnote is the thoroughness of the list of 'important and difficult issues' which they cite and which is backed by citing so many positivistic social scientists. It is resolved by assuming the difficulties to be 'non-problematic' and to proceed with the analysis – and note the pseudo-scientific language – 'while holding measurement issues constant'. They, in short, hold constant all the factors which could make their analysis a nonsense!

## 2. *Mayhill and Allen Home Office Study of Rape (2002)*

As we have seen, the first British Crime Survey found only one attempted rape in the whole of England and Wales. Extremely aware of the limitations of research into sexual victimisation, and facing a barrage of criticism from feminist scholars, researchers at the Home Office in Britain set out to attempt to remedy this by the use of computer assisted self interviewing techniques (CASI). The procedure in the 2000 sweep of the Survey was as follows: the majority of the crime victimisation interviews are conducted as a face-to-face interview with the interviewer reading out the questions and instructions from his or her computer screen and inputting the results directly into their lap top computer. This is called Computer Assisted Personal Interviewing (CAPI). At the end of the CAPI interview, the respondents are invited to complete the self-completion section

(two sections in the 2000 sweep on drugs and sexual victimisation); the lap top is handed to them and the CASI interview begins.

Thus the introduction of the supposedly anonymous technology is supposed to eliminate the embarrassment of the interview situation.

What can we make of this? First of all let us quickly skim over the usual problem of non-response (34% in this instance), that there are differentials by age and class about use of computers, that in a minority of instances the interviewer assisted the interviewee, and that the presence of other people in the room – not only the interviewer – is bound to re-define the interview situation (see Mooney, 2000 for a critique of CASI with respect to domestic violence in the 1999 sweep of the BCS). Let us focus on the more substantive part of the problem, the screening question itself:

“Year Since age 16 has someone, either a stranger or someone you know  
2000 used violence, threats or intimidation to force you to have sexual  
intercourse against your will?

By sexual intercourse we mean either vaginal or anal penetration.

4.5%”

(Myhill and Allen, 2002, p.12)

It must be blatantly obvious that such a question will be subject to a multiplicity of interpretations and must of necessity do so. Indeed on the very next page they note when trying to explain why the 1998 and 2000 survey set the same result despite different screener questions:

‘Perhaps the most interesting thing to note is that, despite wording changes, the prevalence rate derived from the 1998 and 2000 screener questions are virtually identical. Of course it is impossible to know the exact reasons for this. It does perhaps highlight, though, the problems associated with having one or two reasonably subjective screener questions acting as a filter for victims’ experiences. *It is extremely difficult*

*to guess with any accuracy exactly what types of incidents are being recorded, bearing in mind different people's perceptions and subjective interpretations of certain words and phrases.'* (*ibid.*, p.13, italics mine)

But \_\_\_\_\_ we know that rape is an extraordinarily contested area and, of course, the researchers being both well informed and meticulous and well aware of this. They note that the large proportion of rape between intimates make women hesitate as to classification, they register Kate Painter's (1991) finding in her study of married women and rape that some victims were only willing to classify their experience as rape retrospectively after considerable hindsight, and they stress that the concept of 'rape' may carry with it connotations which women may not want to associate with themselves. In short, they are aware of change, resistance, and contest surrounding the notion of what constitutes rape. Despite all of this the hubris of numbers overcame this so that they can boldly state

'the BCS estimates that 4.9 per cent of women have been raped since age 16 and that 9.7 per cent of women have suffered some sort of sexual victimisation since that age ... The 2000 BCS estimates that 0.9 per cent of women suffered some form of sexual victimisation during the last year and 0.4 per cent suffered rape ... [it] estimates that there are approximately 754,000 females aged 16 to 59 in England and Wales who have been the victim of rape once or more since the age of 16. this includes approximately 61,000 victimised in the last year.'

 (*ibid.*, pp.17-19)

I will return to the changing definition of rape later, but let us for the moment reflect on these examples.

The above two examples are selected from the many: they are the stuff of footnotes or asides. Yet they nestle like dynamite in the book or article: they are the cautionary asides which, if read seriously, would destroy the main narrative, they are the momentary truth which in terms of the project should have never even been said. 'Normal science' in the terms of Thomas Kuhn proceeds, of course, as usual – although it really isn't science: the articles mount up, the cross references are made, the careers develop and the knowledge – whatever one is to make of it – accumulates in the mountains of articles and monographs.

What is of interest is the psychological mechanisms which permit this. Let us note at the outset they are very like the techniques of neutralisation famously described by Matza and Sykes (\_\_\_\_), the psychodynamics by which the delinquent loses his moral bonds with society, the excuses which permit delinquency, only in this case we are talking of processes by which a rational person is able to loosen his sense of reality or to metaphorically skate happily on thin ice. (For a parallel process in terms of human rights, see S Cohen, 2001).

### **THE IMPACT OF LATE MODERNITY ON MEASUREMENT**

In the introduction to this essay I noted how in late modernity the causes of crime become loosened from their fixed structural moorings. That the 'same' circumstances become imbued with different meanings. The causes of crime, in the sense of a narrative leading from personal circumstances to crime committed, are more varied; they are in less in a fixed relationship. We have seen in our discussion of measurement how the effects – the outcome of such a narrative, are subject also to differential interpretations. They are part of this constructed narrative of meaning both of the actor in the course of his or her life and in the present context of their relationship to the interviewer in the act of measurement. But here again such a social construction is also subject in late modernity to greater contest and pluralism of definition. So the hidden figure expands and contracts with the values we bring to it. In a pluralist society it is no longer possible to talk of a hidden figure  $x$  with which we can attempt to measure, but a whole series of hidden figures  $x, y, z$  etc.

### **PASSING THE ATTITUDE TEST**

The two strategies of positivism are to attempt to correlate rational circumstances (economic conditions, family \_\_\_\_\_, opportunities for crime, etc.) to actual behaviour, or to relate attitudes to possible behaviour. Here all the previous strictures are of relevance – perhaps more so. Non-respondents will, in all probability, have attitudes which are atypical of respondents. But of respondents themselves measurement of attitudes is surely a most tenuous affair. Who, for

example, is to admit to racism, to belief in the inequality of the sexes, to an aversion to work, to relishing violence? The attitudinal survey almost approximates an attitude test: what is the correct attitude to air publicly? It is thus an attitude to an attitude – a population schooled in multiple choice questionnaires seeks the ‘right’ answer.

Even more of a problem is when our attitude is held with hesitancy, uncertainty or contradiction. Many a strongly contradictory opinion has been registered 3.5 on a 7 point Lichert Scale.

But put all of this in the context of late modernity – there we have a situation of rapid attitude shift, of a media-saturated world where the correct attitudes to affairs of the day is widely and intensely debated. Here is a world of contradiction, of blurring, of boundary testing.

### **THE BOGUS OF POSITIVISM**

The positivist dream of a scientific sociology of crime, which attempts to objectively relate cause and effect, becomes all the more impossible in late modernity. As we have seen, both the causes of crime and the definitions of crime, that is the outcome or effects, become problematised. To move from, say, unemployment to crime, or deprivation to crime, you need narratives; correlation alone cannot assure causality, it is only the narratives which link factors to outcomes that can do this. People turn ‘factors’ into narratives – they are even capable, as we shall see, of turning such factors on their heads. Furthermore, what is crime itself is part of this narrative. It is a variable dependant on subcultural definition and assessment.

The bogus of positivism is that it only *seemed* to work when the world was reasonably static, where vocabularies of motive were seeming organically linked to points in the social structure and where definitions of crime were consensual and unproblematised. The loosening of moorings in late modernity, and the

multiple problematisation consequent on pluralism destroys this illusion. As Martin Nicolaus exclaimed in his famous article in *Antioch Review* so many years ago, 'What kind of science is this, which holds true only when men hold still?' (1969, p.387)

We live in a time of rapid change. In these times, rather than the variables determining the change, it is almost as if the change occurs and the factors seem to scuttle after them. Prediction of real life events of any consequence has always been a lamentable failure in the 'social sciences', just think of the collapse of communism and look at the writings of political scientists prior to the days of *glasnost*. In criminology we have witnessed in our lifetimes two dramatic changes completely contrary to our scientific predictions. First of all, in the period from the sixties onwards, the crime rate increased remorselessly in the majority of industrial countries despite the fact that all the factors which had been identified as reducing crime were on the increase (eg wealth, education, employment, housing). I have termed this elsewhere the 'aetiological crisis' in criminological theory (1994) and this set in motion an intense debate amongst criminologists and is the basis for the extraordinary creativity and plethora of theories that occurred in the last thirty years. But having spent the whole of our professional lives researching why crime should almost inexorably go up (whether by relative deprivation, broken homes, social disorganisation, breakdown of controls, labelling, etc.) we find ourselves in the infuriating position of the crime rate in very many industrial countries (including the US and the UK) begin to go down, against all predictions that I know of. Here we have a double trauma or whammy, if you want!

## **THE CRIME DROP IN AMERICA AND THE CRISIS OF POSITIVISM**

'On November 16 2000, in San Francisco, a packed meeting of the American Society of Criminology gathered together to discuss a most extraordinary happening in the world of crime. For from 1991 onwards, violent crime in the

United States, which had led the advanced world by far in rates of murder and robbery, had begun to fall. Homicide dropped by 35.7 per cent from 1991 to 1998 (from 9.8 to 6.3 per 100,000) (Blumstein and Wallman, 2000). Al Blumstein, of the National Consortium on Violence Research had brought together a dazzling array of experts: demographers, economists, sociologists and criminologists, all contributing their views on the change with graphic charts and probing statistical analysis. I listened with fascination to how they factored each of the developments over the period to explain the phenomenon, from changes in the distribution of handguns, the extraordinary prison expansion, zero-tolerance policing, down to changes in crack-culture and technology. At the end of the session they asked for comments from the audience, no doubt expecting some detailed remark about policing levels or the influence of handgun availability, or such like; but the first question, from a Canadian woman, was something of a revelation. She pointed out, ironically, how Canadians were supposed to be condemned to culturally lag behind their American cousins, but that they too had had a drop in violence, despite the fact that they had not experienced such a period of rapid prison expansion, that zero-tolerance policing was not *de rigueur* and that Canada had only a small problem of crack-cocaine. (see commentary in Ouimet, 2002). She was followed by a Spanish woman, who said something very similar about her country. And, in fact there was a crime drop in 13 out of 21 industrial countries during 1997-98 (Barclay *et al.*, 2001; Young, 2004)

Blumstein's team focused on the relationship between variable changes and the drop of violence, once international data is examined one must seriously question whether they were looking at the correct variables and furthermore, to cap it all, they traced their line of correlation between these variables and the level of violence when, in fact, property crimes were also declining. The most immediate explanation of this is that we are encountering 'spurious causality'. Here again we have an instance of wide scale awareness of the practice of

moving from correlation to causation, coupled with an undue haste to move on over it in the interest of science. As Andrew Sayer puts it:

'To a certain extent, the limitations of 'statistical explanations' are well known and teachers of statistical methods usually have their favourite example of a 'spurious correlation' such as that discovered between the birth rate and the number of storks in different regions of Sweden. The problem is usually acknowledged in a token fashion by placing the terms 'statistical explanation' and 'causal' in scare quotes, but the use of statistical analysis is often intended to *suggest* that the quantitative relations so discovered are causal. Regression equations, for example, say nothing in themselves about causal or conditional relations, yet there is a widespread assumption that 'causal analysis' and regression analysis are virtually synonymous.' (1992, p.193)

But the enigma of the crime drop takes us far beyond the world of technical mistakes. The usual procedure in such analysis is to take the demographics and other factors which correlate with crime in the past and attempt to explain the present or predict the future levels of crime in terms of changes in these variables. The problem here is that people (and young people in particular) might well change independently of these variables. For in the last analysis the factors do not add up and the social scientists begin to have to admit the ghost in the machine. Thus, Richard Rosenfeld of Blumstein's team writes ruefully:

'If the church is the last refuge of scoundrels, "culture" is the final recourse of social scientists in search of explanations when existing economic, social and political theories have been exhausted.' (2000, p.157)

So there we have it, subculture becomes the final refuge of scoundrels! And Rosenfeld comments, 'It is possible that American adults are becoming, in a word, civilised' (*ibid*, p.156). Leave aside the ethnocentrism, because such a thought, usually with reference to Norbert Elias' *The Civilising Process* (1994), seems to be gaining currency worldwide.

From a more sympathetic perspective, Andrew Karmen in his meticulous analysis of the New York crime drop – *New York Murder Mystery* (2000) casts

his eyes across all the various explanations judiciously giving them various explanatory weightings, but at the end of the book talks of 'the final demographic factor which might be the most important of all' (2000, p.249). But then, he reflects, 'the shift is not even strictly demographic in nature: it is attitudinal and behavioral as well as generational' (*ibid*). And, he adds, 'Unfortunately the existence of this suspected evolution in subcultural values defies precise statistical measurement. It is not clear what kind of evidence and statistics could prove or disprove it.' Karmen points to the possibility of profound changes in the norms of urban youth culture. And here he refers to the pioneering work of Ric Curtis, the New York urban anthropologist who talks of the 'little brother syndrome'. That is, where younger children, having witnessed the devastating effects of hard drugs, gun culture, intensive crime on their older brethren decide that these things are not for them, they are no longer hip and cool – the culture evolves and turns its face against the past. This observation has ready resonance with, for example, any attempt to understand changes in drug use. These do not seem to relate to changes in social factors or the impact of the war against drugs. They seem to relate to changes in fashion – although this is perhaps too light a word for it – changes in subcultural project would probably be more fitting.

Curtis relates these changes closely to the development of late modernity, to the loosening of the moorings which I referred to earlier in this chapter. Thus, he writes: 'The postmodern global economy is one which identity formation is less dependent upon the influence of family, neighborhoods, race/ethnicity, nationality and history, and more than anywhere else the inner city is an empty canvass an open frontier where new structures, institutions and conventions are waiting to be built.' (1998, p.1276).

### **AN OPEN SEASON ON NUMBERS?**

Am I suggesting an open season on numbers? Not quite: there are obviously – as Sennett points out in the Sex in America debate – numbers which are

indispensable to sociological analysis. Figures of infant mortality, age, marriage, common economic indicators are cases to point as are, for example, numbers of police, imprisonment rates and homicide incidences in criminology. Others, such as income or ethnicity, are of great utility but must be used with caution. There are things in the social landscape which are distinct, definite and measurable, there are many others that are blurred because we do not know them – some because we are unlikely ever to know them, others, more importantly, which are blurred because it is their nature to be blurred. Precision must be constantly eyed with suspicion, decimal points with raised eyebrows. There are very many cases where statistical testing is inappropriate because the data is technically weak – it will simply not bear the weight of such analysis. There are many other instances where the data is blurred and contested and where such testing is simply wrong. Over the last decade there has grown up a peculiar formula for writing journal articles. The introduction usually presents two theories in competition, but they are strange one-dimensional creatures almost unrecognisable compared to the real thing, by virtue of being rendered simple and decontextualised for the purposes of operationalisation. This acephalous introduction, this headless chicken of an argument, is then followed by an extensive discussion of measures whilst the data itself is usually outsourced from some past study, or bought in from a survey firm, an obligatory recession analysis follows, an erudite statistical equation is a definite plus, and then the usually inconclusive results are paraded before us. The criminologists themselves are far distant from crime out there, hidden behind a wall of verbiage and computer printout, the barrier graphited with the Greek letters of statistical manipulation.

What can we do to get out of this sanitised redoubt? What is needed is a theoretical position which can enter in to the real world of existential joy, fear, false certainty and doubt, which can seek to understand the subcultural projects of people in a world riven with inequalities of wealth and uncertainties of identity. What we need is an ethnographic method which can deal with reflexivity,

contradiction, tentativeness, change of opinion, posturing, and concealment. A method which is sensitive to the way people write and rewrite their personal narratives. Our problems will not be solved by a fake scientificity, but by a critical ethnography, honed to the potentialities of human creativity and meaning.

## BIBLIOGRAPHY

- Barclay, G; Tavares,C, and Siddique, A, 2001, *International Comparisons of Justice Statistics, 1999*. London: Home Office
- Bauman, Z, 2000, *Liquid Modernity*. Cambridge: Polity Press
- Bauman, Z and Tester, K, 2001, *Conversations with Zygmunt Bauman*. Cambridge: Polity
- Blumstein, A and Wallman J (eds) 2000, *The Crime Drop in America*. Cambridge: Cambridge University Press
- Braithwaite, J, 1981, 'The Myth of Social Class and Criminality Reconsidered', *American Sociological Review*, 46, pp.36-57
- Braine, R; Busway, S, and Paternoster, R, 2003, 'Examining the Prevalence of Criminal Desistance', *Criminology* 41(2), pp.423-448
- Crawford, A; Jones, T; Woodhouse, T, and Young, J, 1990, *The Second Islington Crime Survey*. Middlesex University: Centre for Criminology
- Currie, E, 1985, *Confronting Crime*. New York: Pantheon
- Curtis, R, 1998, 'The Improbable Transformation of Inner City Neighborhoods: Crime, Violence and Drugs in the 1990s', *Journal of Criminal Law and Criminology*, 88(4), pp.1233-66
- Downes, D and Rock, P, 1988, *Understanding Deviance*. Oxford: Clarendon Press
- Durkheim, E, 1938, *Rules of the Sociological Method*. New York: The Free Press
- Durkheim, E, 1951, *Suicide*. New York: The Free Press
- Elias, N, 1994, *The Civilising Process*. New York: Blackwell
- Eysenck, H and Gudjonsson, G, 1989, *The Causes and Cures of Criminality*. New York: Plenum Press
- Farrington, D; Joliffe, D; Hawkes, D; Catalano, R; Hull, R, and Kosterman, R, 2003, 'Comparing Delinquency Careers in Court Records and Self-Reports', *Criminology*, 41(3), pp.933-58

- Ferrell, J, 1997, 'Criminological Verstehen: Inside the Immediacy of Crime', *Justice Quarterly*, 14, pp.3-23
- Francome, C, 1993, 'Social Behaviour', *British Medical Journal*, 306, p.573
- Gaines, D, 1998, *Teenage Wasteland: Suburbia's Dead End Kids*. Chicago: University of Chicago Press
- Garland, D, 2000, 'The New Criminologies of Everyday Life: Routine Activity Theory in Historical and Social Context' in A VanHirsch, D Garland and A Wakefield (eds) *Ethical and Social Perspectives on Situational Crime Prevention*. Oxford: Hart Publishing
- Giffen, P, 1965, 'Rates of Crime and Delinquency' in W McGrath (ed) *Crime Treatment in Canada*. Toronto: Macmillan
- Gottfredson, M and Hirschi, T, 1990, *A General Theory of Crime*. Stanford, CA: Stanford University Press
- Hayward, K and Young, J, 2004, 'Cultural Criminology: Some Notes on the Script', *Theoretical Criminology* [vol, page numbers etc.]
- Herlitz, C, 1990, 'Sexual Behaviour in the General Population of Sweden', *Social Science and Medicine*, 36(12), pp.1535-40
- Herrnstein, R and Murray, C, 1994, *The Bell Curve*. New York: The Free Press
- Hough, M and Mayhew, P, 1983, *The British Crime Survey*. London: The Home Office
- Hough, M, 1986, 'Victims of Violent Crime, Findings from the British Crime Survey' in E Fattah (ed) *Crime Policy to Victim Policy*. London: St Martins Press
- Jones, T; McClean, B, and Young, J, 1986, *The Islington Crime Survey*. Aldershot: Gower
- Karmen, A, 2000, *New York Murder Mystery*. New York; New York University Press
- Katz, J, 1994, 'Rights Groups Scorn "1 in 90 gay" Survey', *The Guardian*, 22 January, p.5
- Katz, J, 2002, 'Start Here: Social Ontology and Research Strategy', *Theoretical Criminology*, 6(3), pp.255-78

- Laub, J, 2004, 'The Inter Course of Criminology in the United States: The American Society of Criminology 2003 Presidential Address', *Criminology*, 42(1), pp.1-26
- Laumann, E; Gagnon, J; Michael, R, and Michaels, S, 1995a, *The Social Organisation of Sexuality: Sexual Practices in the United States*. Chicago: University of Chicago Press
- Laumann, E; Gagnon, J; Michael, R, and Michaels, S, 1995b, 'Letter to Editor', *New York Review of Books* 42(9) (May), p.43
- Lewontin, R, 1995a, 'Sex in America', *The New York Review of Books* 42(7) (April 21<sup>st</sup>), pp.24-29
- Lewontin, R, 1995b, 'Letter to Editor', *The New York Review of Books*, 42(9) (May), p.43-4
- Maguire, K and Pastore, A, 2001, *Sourcebook of Criminal Justice Statistics 2000*. Washington: Bureau of Justice Statistics
- Michael, R; Gagnon, J; Laumann, E and Kolata G, 1995, *Sex in America: A Definitive Survey*. Boston: Little Brown
- Mirlees-Black, C, 1999, *Domestic Violence: Findings from a New British Crime Survey*. London: HMSO
- Mirlees-Black, C; Mayhew, P, and Percy, A, 1996, *The 1996 British Crime Survey*. London: The Stationery Office
- Mooney, J, 1999, *Gender, Violence and the Social Order*. London: Macmillan
- Myhill, A and Allen, J, 2002, *Rape and Sexual Assault of Women: The Extent and Nature of the Problem*. London: Home Office
- Nicolaus, M, 1969, 'The Professional Organization of Sociology: A View from Below', *Antioch Review* (Fall), pp.375-87
- Oosthoek, A, 1978, *The Utilization of Official Crime Data*. Ottawa: Supply and Services
- Ouimet, M, 2002, 'Explaining the American and Canadian Crime "Drop" in the 1990's', *Canadian Journal of Criminology*, 44, pp.33-50
- Painter, K, 1991, *Wife Rape, Marriage and the Law*. Manchester: Faculty of Economic and Social Studies, University of Manchester

- Plummer, K, 1995, *Telling Sexual Stories*. London: Routledge
- Quetelet, A, 1842, *Treatise on Man*. Paris: Bachelier (first published in 1835 as *Sur l'homme et le développement de ses facultés ou Essai de physique sociale* 2 volumes)
- Rosenfeld, R, 2000, 'Patterns in Adult Homicide: 1980-1995' in A Blumstein and J Wallman (eds) *The Crime Drop in America*. Cambridge: Cambridge University Press
- Sayer, A, 1992, *Method in Social Science*, 2<sup>nd</sup> ed. London: Routledge
- Schwendinger, H and Schwendinger, J, 1985, *Adolescent Subcultures and Delinquency*. New York: Praeger
- Sennett, R, 1995, 'Letter to Editor', *New York Review of Books* 42(9) (May), p.43
- Skogan, W, 1986, 'Methodological Issues in the Study of Victimization', in E Fattah (ed) *Crime Policy to Victim Policy*. London: St Martins Press
- Sparks, R F, 1981, 'Surveys of Victimization – An Optimistic Assessment' in M Tonry and N Morris (eds) *Criminal Justice* Vol.3. Chicago: University of Chicago Press
- Sparks, R F; Genn, H, and Dodd, K, 1977, *Surveying Victims*. London: Wiley
- Taylor, I; Walton, P, and Young, J, 1973, *The New Criminology*. London: Routledge and Kegan Paul
- Tittle, C; Villemez, W, and Smith, D, 1978, 'The Myth of Social Class and Criminality', *American Sociological Review*, 43, pp.643-56
- vanKesteren, J; Mayhew, P, and Nieuwbeerta, P, 2000, *Criminal Victimization in 17 Industrialized Countries: Key Findings from the 2000 International Crime Survey*. The Hague, NL
- Young, J, 1988, 'Risk of Crime and Fear of Crime: A Realist Critique of Survey-Based Assumptions', in M Maguire and J Pointing (eds) *Victims of Crime: A New Deal*. Milton Keynes; Open University Press
- Young, J, 1994, 'Incessant Chatter: Recent Paradigms in Criminology', in M Maguire, R Morgan and R Reiner (eds) *The Oxford Handbook of Criminology*, 1<sup>st</sup> ed. Oxford: Oxford University Press

Young, J, 2003, 'Winning the Fight Against Crime' in R Matthews and J Young (eds) *The New Politics of Crime and Punishment*. Cullompton, Devon: Willan